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intergrades are possible. If, on the contrary, the characteristic refuses to blend, but comes out of the cross intact, as it went in, the conclusion seems justified that the characteristic is essentially integral and must have arisen completely formed, and hence discontinuously.

Using this criterion, I have of late been testing the application of the mutation theory to animals and have had an opportunity to examine the experiments of others. Some of the work has been done on the characteristics of domesticated 'races,' others on wild varieties. There seems to be no difference in the behavior of characteristics of domesticated and wild varieties. The result is that most characteristics, but not all, fail to blend and are strictly alternative in inheritance. I interpret this to mean that the characteristic depends on a certain molecular condition that does not fractionize. The inference is that if the characteristic is incapable of gradations now it has always been and hence must have arisen without gradations, *i. e.*, discontinuously. Examples of such discontinuous characteristics are the spots on the elytra of certain beetles, the crest on the canary, the form of the comb in poultry, extra toes, black plumage and color of iris. One who sees the striking failure of these characteristics and many others to be modified in any important way will feel convinced that they are not capable of forming intergrades and hence could not have arisen gradually.

While I am not of those who would seem to deny that characters of domesticated species are as *natural* as any others, it is worth inquiring whether discontinuous variations, such as I have been dealing with, occur among feral animals. The evidence is that they do. Thus our gray squirrels exhibit in many localities a striking number of black individuals. These are not found everywhere, but in small areas may be fairly common. Difference in climatic conditions can not account for the blacks—they belong to the order of melanic sports, *i. e.*, mutations. Our red squirrels and various other feral rodents sport in the same way. Birds also show melanic sports, *e. g.*, in the European snipe (*Scolopoc-*

gallinago) a chocolate brown form sometimes appears which, like the black squirrel, has been considered by some as a distinct species. Similarly, more or less albinic sports occur in nature. White crows and blackbirds are well known and many individuals of the house sparrow are partially albinic. The history of the twisted beak of the crossbill (*Loxia curvirostra*) is, of course, unknown. The characteristic is, however, the same as, and has probably had a similar origin with, that which suddenly appears in one per cent. of the poultry that I breed and which has been observed as a sport in crows. Scarcely one of the characteristics of poultry may not be found appertaining to some feral species, and there is every reason to believe that these characteristics have the same property of indivisibility in the latter case as in the former. Such facts as I have cited above could be added to by Dr. Merriam or any other naturalist with a similarly extensive and profound knowledge of the higher vertebrates; and they seem to me to lead to the conclusion that some new characters may arise in nature suddenly, as sports or mutations, and persist as specific characteristics.

CHAS. B. DAVENPORT.
STATION FOR EXPERIMENTAL EVOLUTION,
October 18, 1906.

THE RIGIDITY OF THE EARTH.

TO THE EDITOR OF SCIENCE: In SCIENCE of September 28 Professor L. M. Hoskins is led to the sad conclusion that I have misunderstood Lord Kelvin's definition of the modulus of rigidity, and he thus apparently questions the results which I have given in *Astronomische Nachrichten*, No. 4,104. Owing to the great length of that paper, my explanation of the connection between rigidity as experimentally determined for solid bodies here upon the earth's surface and other bodies kept rigid by pressure was not sufficiently developed; and as the difficulty that has misled Professor Hoskins appears to have occurred also to others, it seems worth while to point out the omitted steps in the chain of reasoning, which will, I think, make it clear that *my process* has been misunderstood and misinterpreted,

rather than Lord Kelvin's, which has been familiar to me for many years.

1. The error which has arisen in judging my paper proceeds from the habit of dealing with common solids in the laboratory, and the supposition that I am using the same method in dealing with the effective rigidity of the matter within the earth. The question as to how the stresses are applied to a cubical element does not need to be considered, for we are not experimentally shearing or otherwise deforming the elemental cubes of the earth to get the resulting mean rigidity. Inside the limit of pressure which gives the matter the property of an elastic solid, the simple fact is that there is an effective rigidity in spite of the high temperature. Pressure operating through the agency of molecular forces, therefore, is the sole cause of the effective rigidity and I have taken the effective rigidity everywhere proportional to the pressure, which is a perfectly legitimate hypothesis. If others wish to adopt a different hypothesis, they are at liberty to do so. The present hypothesis is satisfactory on theoretical grounds, and apparently confirmed by the numerical calculations given in *Astronomische Nachrichten*, No. 4,104.

2. It may be well to observe that it is a matter of the utmost indifference to me how the elemental cube may be distorted, or whether it be distorted at all. I am not determining coefficients of rigidity for the different elements within the earth. *For my purpose of calculating the earth's mean rigidity, it is sufficient to have something which these rigidity moduluses would be proportional to if they could be determined, and that is the pressure, as calculated from the theory of gravity and Laplace's law of density.*

3. The rigidity of ordinary solids may be expressed in atmospheres; and in dealing with bodies made rigid by pressure, it is convenient to employ the same measure, since this enables us to compare the rigidity of a cold solid to that of a hot body made rigid by confining pressure.

4. There is an old saying that 'facts are stubborn things.' Such, it seems to me, are

the numerical results obtained in my paper, by processes of entire mathematical rigor. I calculate that the rigidity of the earth will lie between 750,000 and 1,000,000 atmospheres. In finding this lower limit, the effect of the earth's crust is neglected, and there is, moreover, some slight defect in the gravitational method near the surface even in the case of encrusted bodies. In the case of gaseous bodies, the outermost layers can hardly be regarded as having the properties of an elastic solid, and hence the integration for the mean pressure should stop before we reach the surface. But as we do not know at what depth to stop, I took the mean pressure of the entire planet as giving its most characteristic property.

From these considerations I believe that those who study the paper in *Astronomische Nachrichten*, No. 4,104, will agree that the points raised relate to the experimental determination of moduluses of rigidity, and not to the rigidity of the earth and other planets, which are found by theoretical methods fully explained in the paper itself.

T. J. J. SEE.

U. S. NAVAL OBSERVATORY,
MARE ISLAND, CALIF.,
October 3, 1906.

ANATOMIC NOMENCLATURE: AN OPEN LETTER TO
PROFESSOR LLEWELLYS F. BARKER.

Dear Dr. Barker: Through absence from home I have but just received from the publishers your "A Description of the Basle Anatomical Nomenclature [B N A], advance sheets from Dr. Llewellys F. Barker's forthcoming book, 'Anatomical Terminology.'" I rejoice that the subject is to be so fully and ably presented to English-speaking teachers and students of anatomy. Although many of the terms of the [B N A] are not preferred by me, yet—pending the expected eventual general acceptance of my own—I should hail their provisional adoption to the exclusion of their numerous even less worthy synonyms, as enabling me to replace a 'shot-gun policy' by rifle-practise.

I take for granted that the paragraph on page 5 was intended to represent justly my